REVIEWER 1 RESPONSE

COMMENT:

Assessing the impact of meteorological forcing on simulated water and energy budgets particularly in mountainous catchments is important. While authors have addressed an important topic, the study falls short on performing a comprehensive quantitative model evaluation compared to observations. Furthermore, broader implication of the results have not been discussed.

REPLY:

Dear reviewer we are very thankful to you for reviewing the manuscript and highlighting the importance of our study. Your valuable comments will certainly improve the quality of our manuscript. As you mentioned, we tried to explain the importance of meteorological distribution methodology throughout our manuscript using a sensitivity study of meteorological distribution on water budget, and we tried to incorporate all possible experiment related to our hypothesis.

However, concerning statistical estimations that have not been included in the study, we will add them in the revised manuscript, the different statistical metrics (slope, correlation, RMSE and MSE) for albedo (table below present values to be written in the new manuscript, **Review Table 1**), evapotranspiration (**Revised Figure 7** below) and snow comparison with the observed data (snow/no-snow surface ratio, **Revised Figure 10** below) is included with our response. Unfortunately, we don't have streamflow data for the selected period but we believe that this is not necessary for the objectives and conclusions of this study. We documented the soil parameters according to field experiment and geophysical survey data which is explained in the text. Their values and distribution do not give us large ranges to tune the model. A companion paper to be submitted soon will discuss more the sensitivity with respect to subsurface configuration and calibration against updated observations including some discharge data.

COMMENT:

Authors performed a limited sensitivity experiment at a small catchment scale to assess the impact of distributed meteorological forcing on simulated water and energy budgets. Although authors performed a number of scenarios, results mostly analyzed qualitatively without providing further insights.

Authors have mostly used qualitative assessment to compare simulation results. Further quantitative assessment of catchment average and spatially distributed data are needed to understand the impact of spatial heterogeneity of meteorological forcings.

REPLY:

Dear reviewer, we agreed that our manuscript lack from quantifications. In our revised manuscript we will include statistical differences among the different simulations to better quantify the intrinsic and relative changes in hydrological budgets. Slope, correlation coefficients and RMSE have been added for evapotranspiration on each plot of **Revised Fig. 7.** Similarly the statistics for albedo and sentinel images has been also included in the attached figure/tables (**Review Table 1, Revised Figure 10).** These all statistics will be further added in the final draft of the manuscript.

COMMENT:

Model evaluation against observed evapotranspiration showed that all model scenarios overestimate ET. Therefore, no further improvement has been achieved by using distributed forcing.

REPLY:

Dear reviewer, we thank you for highlighting this issue. First it has to be noted that the simulated evapotranspiration in the 1D run (uniform meteorological forcing) overestimates ET observations (**Figure 7**). The main reason is that for these simulations we don't take into account solar angle for the available radiation for melting. However, it is obvious on **figure 7** that the representation of evapotranspiration is much better when we distribute the shortwave radiation (2D-AD and 2D-SD simulations) which take into account solar angle and catchment slope. At the contrary, this is not the

case when only precipitation or only wind is distributed. This is one of the major highlight of our manuscript. We added some statistical estimates to better quantify the improvement in evapotranspiration among different simulations. Another reason to overestimate the evapotranspiration could come from our footprint area. We have one meteorological observation at flat surface and the catchment is very undulating. Though we presented a wind direction mask (**Figure 1**) to consider large heterogeneity (moisture, vegetation) in the ET observation footprint and better compare evapotranspiration series. It is not as good as in representing the actual footprint area and this can lead to differences between observation and simulations. However, this is not the purpose in this study to have the best comparison for ET which would require a complete footprint area calculation over complex terrain. This is a challenge by itself and these remarks will be added as a separate discussion part in the revised manuscript.

COMMENT:

The thickness of the last subsurface layer is 110 m. At this resolution, groundwater system is simulated as a single reservoir. I wonder why authors needed to use ParFlow.CLM for such a simple parameterization of the subsurface. I agree with authors that simulating lateral flow processes in these steep catchments are important. However, they did not show sensitivity of lateral flow simulations in their simulation scenarios.

REPLY:

Dear reviewer we thank you for this very relevant comment. We started setting up our model with the shallow subsurface (20m) which led to an unsaturated domain everywhere except at the very bottom of the catchment close to the outlet. This means that we were badly simulating transmissivity and potential underground flows associated with the unsolved saturated zone (from top bottom and from side to stream). Hence, to better account the saturation transition from deep subsurface to shallow subsurface from a better solved pressure field, we have increased the depth of the domain. With this configuration we simulate explicitly the saturated zone on more than half of the catchment (**Review Figure 1**). It has to be noted that Parflow suppose hydrostatic profiles within a single mesh, which means that water table depth can be interpolated from solved pressure/suction values in the lower meshes. In our case it could be easily seen as the transition from deep subsurface saturation to shallow subsurface saturation along the mid of the watershed. Finally, the model water velocity outputs are used for running the particle transport model EcoSLIM and calculating the residence time with this configuration is better adopted to account both surface and deep flow path in the catchment.

Last important remark is that the domain has a no-flow boundary condition on the sides and at the bottom of the domain. ET and streamflow outlet are the only way to get the water out. In other words, this means that we are not simulating larger scale flow path (water that enters from the sides of the domain or that gets out through the bottom of the domain).

Also as you said, we do have lateral flows simulation for our catchment for different layers. However the particle tracking simulations we did, showed very low contribution from deep underground flows to streamflow or to ET. Then, in this manuscript we put more emphasis on surface processes impacted by meteorological distribution forcing to catch the spatial snow variability and associated hydrological fluxes. Subsurface sensitivity and its impact on lateral flows will be discussed in detail in our companion manuscript. These remarks will be added in a separate discussion part.

COMMENT:

Given the small size of the catchment, it is difficult to use existing gridded meteorological products to assess the impact of distributed forcing on simulated results. However, given the size of the model, authors could expand the extent of their sensitivity analysis and perform additional scenarios.

REPLY:

Dear reviewer as you said we agree that the size of catchment does not allow to use the gridded meteorological products for sensitivity analysis. ParFLOW-CLM is a critical zone physically based model which allow us to be very close to hydrological processes. This requires reliable data for forcing, ground, vegetation and hydrology to keep consistency in the model framework to simulate all water path with the same accuracy. We then chose to work only with local observations from which we built distributed forcing based on published algorithm and evaluate the model. From this approach we can clearly see the importance of snow and incoming radiation distributions.

Adding extra sources of data would have brought confusion to the message we wanted to carry with this study. Indeed, using reanalysis data could have led to better simulation of ET or streamflow scores but might be for wrong reasons. It can be good for the elevation at which our catchment lies but may not be as good for few hundred meters apart for different slope orientation or different micro-topography. It has also to be noted that these reanalysis products are much coarse in mountainous regions.

However, note that in our discussion we refer to Y. Fan et al. 2019 who have highlighted that slope/aspect has to be accounted for Earth System Models especially if their resolution is decreasing. We believe our study contribute to that identified issue.

SPECIFIC COMMENTS:

COMMENT:

State in the abstract that the impact of precipitation, wind and shortwave radiation were explored.

REPLY:

Thank you so much for pronouncing this statement. We will add it as "This study explores the impact of precipitation, shortwave radiation and wind speed on the water budgets"

COMMENT:

Line 75 – This is the common land model not the Community Land Model

REPLY:

Sorry for this mistake. We will correct it.

COMMENT:

Line 225 – How did you assess equilibrium state?

REPLY:

The equilibrium state in the model is reached through the 10 years long spinup. The equilibrium is assessed by subtracting the subsurface storage of previous year (Y) from the advancing year (Y+1).

$$\Delta Subsurface\ storage = Subsurface\ storage(Y+1) - Subsurface\ storage(Y)$$

The mean difference for the first year was 50 mm which became -2.2 mm at the end of 10^{th} year reaching the equilibrium state in the model.

COMMENT:

Line 255 – Please change "subsurface stock" to "subsurface storage".

REPLY:

Thank you for highlighting this, we will change it at every instance.

COMMENT:

Line 230 – Differences among various simulation scenarios are not entirely clear. Please clarify.

REPLY:

Thank you so much for mentioning this issue. We will add our attached table (**Review Table 2**) as a separate table in the revised manuscript. This table includes what is distributed and what is not distributed among different simulation.

COMMENT:

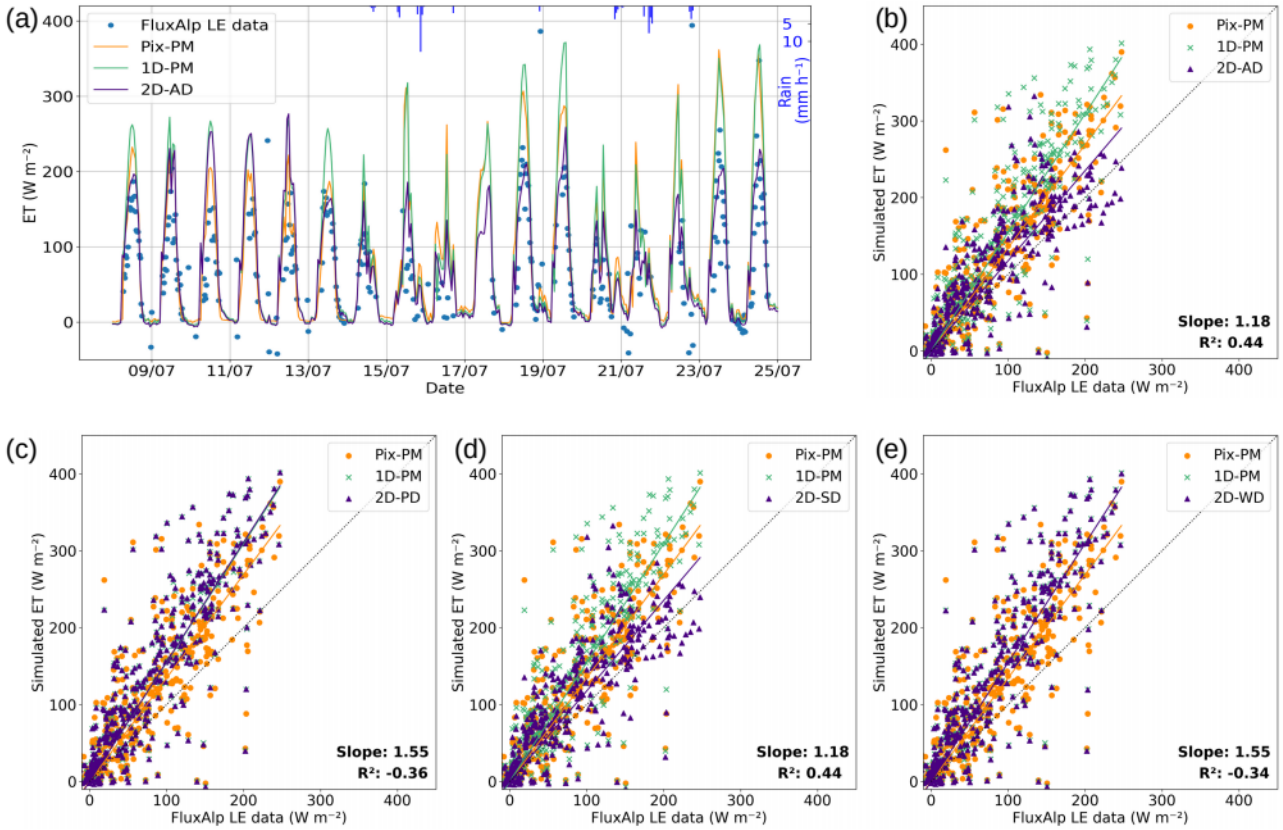
Do you have any runoff observations in this watershed?

REPLY:

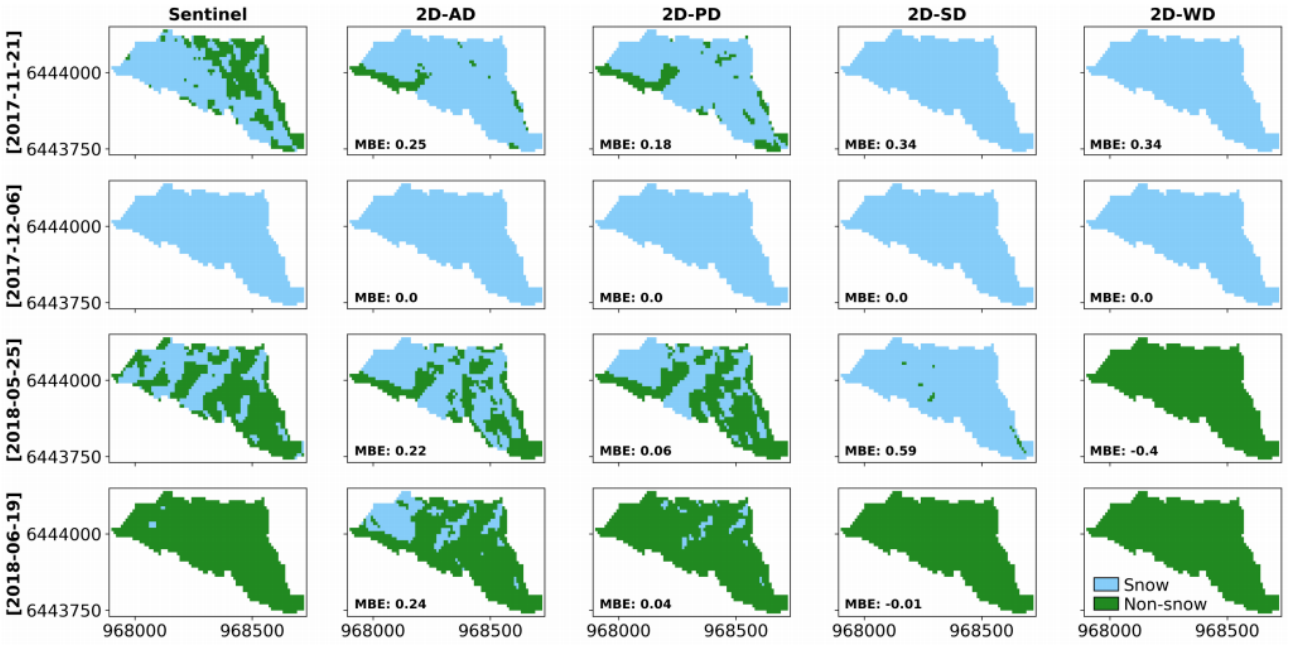
Dear reviewer we do not have any runoff for the year 2017-2018, the year selected for this study. The runoff measurement in the catchment started from spring 2020. As of now we do have a calibrated model for ground parameters. However, as already said above, soil model parameters (Vangenuchten) has been documented from observations we have in our catchment: pedological survey, permeability survey at surface, the underground investigation (accessibility in a tunnel below the catchment) and electromagnetic/GPR survey. This do give us much window to tune our model, it may not be perfect but not far from reality.

Review Table 1: Statistical metrics for observed and simulated parameter among different simulations.

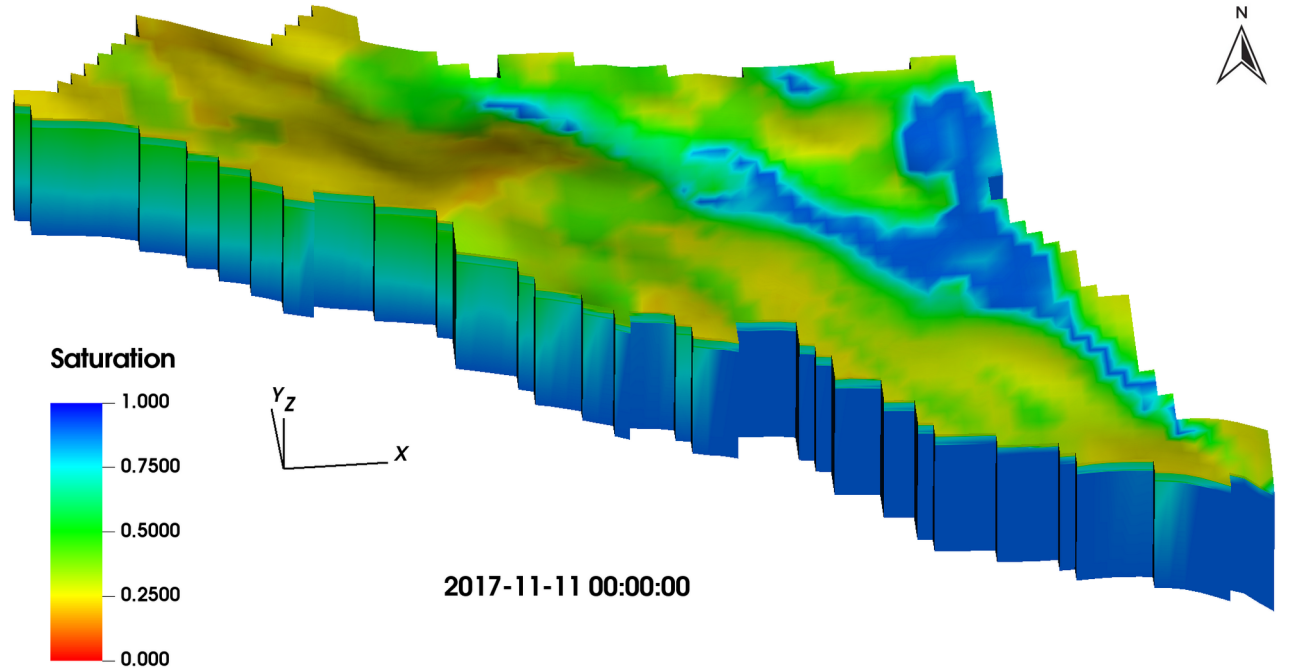
Variable	**Metrics**		**2D-AD**	**2D-PD**	**2D-SD**	**2D-WD**
Evapotranspiration	Slope		1.18	1.55	1.18	1.55
	R-Square		0.44	-0.36	0.44	-0.34
	RMSE		50.77	79.14	50.90	78.41
Albedo	R-Square		0.85	0.88	0.77	0.85
	RMSE		0.12	0.10	0.14	0.12
Snow cover (Sentinel2)	MBE	21 Nov, 2017	0.25	0.18	0.34	0.34
		06 Dec, 2017	0.00	0.00	0.00	0.00
		25 May, 2018	0.22	0.06	0.59	-0.40
		19 June, 2018	0.24	0.04	-0.01	-0.01
	RMSE	21 Nov, 2017	0.63	0.65	0.58	0.58
		06 Dec, 2017	0.00	0.00	0.00	0.00
		25 May, 2018	0.74	0.75	0.78	0.63
		19 June, 2018	0.50	0.23	0.07	0.07



Revise Figure 7: (a) Evapotranspiration simulation masked with wind direction mask for 17 days in summer for all distributed run (2D-AD). Scatter plot in the same month for (b) all distributed run (2D-AD), (c) only precipitation distributed run (2D-PD), (d) only shortwave radiation distributed run (2D-SD) and (e) only wind distributed run (2D-WD).



Revise Figure 10: Snow map for different simulations compared with the Sentinel-2 images for 4 cloud free images: snow pixels (light skyblue) and non-snow pixel (green).



Review Figure 1: Saturation after spinning up model for 10 years. The saturated zone in upper half of the catchment is much deep compared to lower half of the catchment.

Review Table 2: Distributed and non-distributed approach adopted for different simulation.

	Precipitation	**Shortwave radiation**	**Wind speed**
Pix-PM	Distributed mean	Non-distributed	Non-distributed
1D-PM	Distributed mean	Non-distributed	Non-distributed
1D-AM	Distributed mean	Distributed mean	Distributed mean
2D-AD	Distributed	Distributed	Distributed
2D-PD	Distributed	Non-distributed	Non-distributed
2D-SD	Distributed mean	Distributed	Non-distributed
2D-WD	Distributed mean	Non-distributed	Distributed

REVIEWER 2 RESPONSE

GENERAL COMMENTS

COMMENT:

The aim of this paper was to explore the impact of using spatially distributed meteorological forcing (precipitation, solar radiation and wind) on the simulated water and energy balances, such as simulated snow cover dynamics, evapotranspiration, runoff, etc. on a small (15 ha) mid elevation French catchment. The coupled hydrology-land surface model, ParFlow-CLM with a hyper-resolution of 10 m was used in this study. It was found that accounting for spatially distributed precipitation had the largest influence on the catchment's hydrological behaviour. The study demonstrated the importance of accounting for the influence of the terrain when hydrologically modelling a mid-elevation catchment.

This is an interesting paper, the presented methods and conclusions of the study could be useful for the hydrologic modelling community.

A few comments addressing methodological and organizational issues, as well as minor technical corrections are listed below.

REPLY:

Dear reviewer firstly we would like to thank you for such a rigorous review and highlighting the importance of our manuscript. We are definitely sure that your comments will improve the quality and readability of our manuscript. We will try to incorporate your suggestions in all possible means to advance our manuscript.

SPECIFIC COMMENTS

COMMENT:

Currently, only results of the model calibration is presented. It would be good, if the model could be tested on a different, independent time period. It would be also interesting to see if the results/conclusions remain the same for an independent time period.

REPLY:

Dear reviewer, we first want to precise that we didn't follow statistical calibration procedure as it is usually performed with other hydrological model. With ParFLOW, the "calibration" itself consist in building the model, which means underground geometry and parameters from observation were only used. As we don't have observations for each pixels, we also built the model on assumptions when we suppose that what we measure is also valid for places where we do not measure. Model parameters are then not supposed to change intrinsic to the geological characteristics of the catchment but with climate variability. This restrict us for broader ranges to tune the model as the model is forced with observed precipitation and solve explicitly melting and evapotranspiration following basic physical laws. Later the model has been evaluated against the radiation budget observation (albedo), Energy budget observations (Evapotranspiration), snow coverage, ability of the model to produce baseflow and snow melt timing. We will add a paragraph in the model setup section that precise clearly the constraints imposed by the model. This will also be an explanation to the next comment along with new tables and figures added to account our simulations statistically (**Review Table 1, Revised Figure 7 and Revised Figure 10**).

COMMENT:

I am not entirely sure for which field measurements the model was calibrated? Actual evapotranspiration was overestimated for each scenario. If there are observed latent heat flux time series available, would it be possible to change some e.g. model parameters in order to more realistically simulate actual evapotranspiration? It is interesting to see that certain modelling states became e.g. more patchy, or that by accounting for spatially distributed precipitation some of the simulated model fluxes and state variables changed – but how can the authors decide if the modelling results are realistic or better compared to model setup where meteorological forcing is not distributed?

REPLY:

Dear reviewer, as we stated above we will add a short paragraph to explain with what data we evaluate model performances.

Concerning evapotranspiration, first it has to be noted that the simulated evapotranspiration in the 1D run (uniform meteorological forcing) overestimates ET observations (**Figure 7**). The main reason is that for these simulations we don't take into account solar angle for the available radiation for melting. However, it is obvious on **figure 7** that the representation of evapotranspiration is much better when we distribute the shortwave radiation (2D-AD and 2D-SD simulations) which take into account solar angle and catchment slope. At the contrary, this is not the case when only precipitation or only wind is distributed. This is one of the major highlight of our manuscript. We added some statistical estimates to better quantify the improvement in evapotranspiration among different simulations. Another reason to overestimate the evapotranspiration could come from our footprint area. We have one meteorological observation at flat surface and the catchment is very undulating. Though we presented a wind direction mask (**Figure 1**) to consider large heterogeneity (moisture, vegetation) in the ET observation footprint and better compare evapotranspiration series. It is not as good as in representing the actual footprint area and this can lead to differences between observation and simulations. However, this is not the purpose in this study to have the best comparison for ET which would require a complete footprint area calculation over complex terrain. This is a challenge by itself and these remarks will be added in a separate discussion part.

We were showing in our figure 7, 9 and 10 that the simulations run for distributed precipitation and shortwave radiation (adopted in 2D-AD simulation) are closer to the reality. Indeed, neither of 1D forcing nor individual 2D distributed forcing (2D-PD, 2D-SD and 2D-WD) simulations were able to simulate relevant ET and relevant snow patchiness together. However, even if a better tuning could be found especially when it will be evaluated with streamflow data, the paper shows that it is always necessary to account for slope effects especially on radiation and on precipitation distribution. These remarks will be added in a separate discussion section.

COMMENT:

Furthermore, it would be good if the simulation efficiency of the model in terms of simulating snow cover (Figure 10) could be quantitatively assessed, not only visually comparing the simulation results with (only) four Sentinel-2 images – how good are the snow cover simulations during the rest of the year?

REPLY:

Dear reviewer thank you so much for highlighting this issue. Concerning statistical estimations that have not been included in the study, we will add them in the revised manuscript, the different statistical metrics (slope, correlation, RMSE and MSE) for albedo (table below present values to be written in the new manuscript, **Review Table 1**), evapotranspiration (**Revised Figure 7** below) and snow comparison with the observed data (snow/no-snow surface ratio, **Revised Figure 10** below) is included with our response.

For the snow figures we chose sentinel images that belong to the accumulation and the melting period (2 images of each). This choice is justify by the fact that from these images we can only evaluate the patchiness but not the snow depth. Snow depth time series is compared to simulation in **figure 9** and it corresponds to spatial range and mean values. However, this cannot be considered as a fully independent evaluation as the same data helped us to build the snow coefficient map shown in figure 1 which is used to distribute the forcing. Sentinel images are completely independent. As we said in the text, these images, during accumulation and melting period show that simulations are very close to observation in terms of onset and offset of snowmelt and in terms of spatial distribution when slope effect are considered in precipitation and solar radiation forcing. To reinforce this we added the quantified relative distribution (patchiness) of snow on **Revised Figure 10**.

COMMENT:

It would be also very good if the authors could formulate the take home messages of the paper in a very clear way, trying to be specific. Instead of writing "that impacts the hydrology" in the Abstract, the authors could be clear which processes are actually influenced? This applies not only to the abstract, but also to the results and conclusion sections.

REPLY:

Dear reviewer thank you for mentioning this drawback of the manuscript. We will include this in the revised version. It will include emphasizing on our terrain based distribution algorithm along with importance of different meteorological distribution i.e.:

- Most of hydrological processes are slope dependant but it is merely taken into account in land surface and hydrological models. The study quantifies the hydrological impacts in term of melting, streamflow and

evapotranspiration dynamic when taking into account, or not, the slope effect. Considering critical zone models applied to mountainous area, we believe that it is mandatory to consider sub-grid-scale slope/aspect effects in large scale models especially when they are used for hydrological studies.

This will be explicitly added in the abstract and in conclusions.

COMMENT:

The study lacks a Discussion section – Lines 394-400 probably try to put the study into the context of existing literature, but only two studies are mentioned. Generally, it would be good to add a separate, very clear Discussion section to the manuscript. Here, the authors could also elaborate on the question, whether their findings are general/could be generalized (different catchments, different climates, etc.)?

REPLY:

Yes, we admit that we have not put the enough discussion on our manuscript. We will add a separate section for discussion with appropriate references. This section will contain all the explanation we discussed with reviewers with appropriate references.

COMMENT:

I found it hard to follow the different simulation setups through the manuscript. There is a list in Lines 227-234, but when the Results are presented it would be very helpful for a reader to sometimes explain in detail the differences between the simulations, to remind the reader what the abbreviations refer to, etc.

REPLY:

Thank you so much for mentioning this issue. We will add our attached table (**Review Table 2**) as a separate table in the revised manuscript. This table includes what is distributed and what is not distributed among different simulation.

COMMENT:

On a similar note, it is hard to understand the figures, and generally the sequence and subsections of the Results. There are a lot of very similar figures and the differences can be often visually not noticed. Often there is jumping between figures, between the beginning and end of the results, e.g. Line 265 already mentions figure 9; Line 300, etc. The Results section could be potentially reorganized or the section names could be changed, i.e. the Results section starts with describing non-distributed and distributed forcing simulations – which probably refers to different simulation setups. But then it continues with sections such as water budget and snow dynamics – I am not sure how these subsections are logically linked.

REPLY:

We have chosen to first describe the drawback of simulations where we do not distribute the forcing which is a usual practice by modeling group. Then we moved to describe the benefits of distributed forcing. We first started with comparing the surface fluxes, then we moved to surface-subsurface fluxes and finally ended the manuscript with importance of simulating the snow cover. We admit that we have repeated figure 1 and figure 9 caption in our early and later statement. We will try to minimise these repetition in the revised manuscript. Furthermore, we will have a look to reorganize the section according to your comment. In the new manuscript we will suppress reference to figures that are to far and report remarks that require a global vision in the discussion part.

We have also added metrics regression lines, correlation coefficients, RMSE and MBE on the figures that will help to support argumentation and readability of figures.

COMMENT:

I think the study might benefit from a very thorough English language editing, some formulations and sentences are unclear (e.g. storage instead of stock?, shortwave radiation instead of writing simply shortwave?, etc.). Please find below, under technical corrections a few suggestions.

REPLY:

Dear reviewer thank you so much for your careful reading and highlighting all these typing/vocabulary mistakes and provided us with suggestions We will replace the stock and shortwave at every instance. We will also check any kind of further correction needed to enhance the readability of the manuscript.

TECHNICAL CORRECTIONS

-Generally: it might be good to use either present or past tense when presenting the methods, results, etc. – but not to mix the two.

REPLY: Dear reviewer we will have a keen look on this issue and use present/past tense.

-Introduction (and also later in the manuscript): generally, it might be better to split very long sentences into more, shorter ones. This might make it easier for the reader to understand and follow the paper.

REPLY: We will correct this issue throughout our manuscript.

-Title: dynamics? Instead of dynamic? Is the word “induced” necessary in the title?

REPLY: Thank you so much for this comment. We will change it as “Impact of distributed meteorological forcing on snow cover and simulated hydrological fluxes over a mid-elevation alpine micro-scale catchment”.

-Line 3: These impact (or influence)

REPLY: We believe that ‘impact’ will be more appropriate word here. Because meteorological distribution clearly changes the hydrological budget.

-Line 9: 3D simulations of what? Please add. Please be specific.

REPLY: This will be changed as “These include 3D simulations of hydrological fluxes with spatially distributed forcing of precipitation, shortwave radiation and wind speed compared to 3D simulations of hydrological fluxes with non-distributed forcing”.

-Line 13: induces

REPLY: We will correct it as below.

-Line 14: please consider to rephrase, “Distributed forcing induces a snowpack” – what does this actually mean? Which forcing? Is “induce” the right word here?

REPLY: We will correct it as “Distributed forcing leads to spatially heterogeneous snow-cover simulation, which becomes patchy at the end of the melt season and shows a good agreement with the remote sensing images (MBE=0.22)”.

-Line 15: what does “good agreement” mean? Please revise, please add some quantitative information.

REPLY: We will add statistical metrics as above in the text and abstract to quantify results.

-Lune 15: what does a “smoother hydrological response” mean? This sentence is unclear. Please consider to rewrite this sentence.

REPLY: We will correct it as “This asynchronous melting results in a longer melting period compared to the non-distributed forcing, which does not generate any patchiness (MBE = 0.59, -0.4)”.

-Line 16: but how can the authors decide if the “patchiness” is more realistic for distributed meteorological forcing compared to non-distributed one? Again, this should be quantitatively proved.

REPLY: We will clarify this sentence as “Among the distributed meteorological forcings tested, precipitation distribution, including snow transport, has the greatest impact on spatial snow cover (MBE = 0.06) and runoff”.

-Line 16: Among

REPLY: We are sorry for this mistake, we will correct it as above.

-Line 17: “impacts the hydrology” – please revise, please be specific, what exactly is influenced and how? And how do the authors know that these results are realistic?

REPLY: We will add mathematical numbers to prove the more realistic simulations as above.

-Line 17: please add: most important in terms of what?

REPLY: “Most important in terms of snow-cover, evapotranspiration and longer melting response”, we will rephrase this sentence as above”.

-Line 19: please add: what does “it” mean? Please also revise the sentence, what does “small differential melting effect” mean? What does “small” mean? How small? This should be quantitatively expressed.

REPLY: The will be revised as “For the primarily east facing watershed studies, the distribution of shortwave radiation adds a small differential snowmelt with an increase in mean bias error (0.06 to 0.22) for all distributed forcing simulations compared to the simulation with only distributed precipitation.”

-Line 20: please remove “participate to”. Please replace “accelerate” with “accelerates”

REPLY: We will correct it according to your suggestions.

-Line 21: patchiness in what? Please add. How do the authors know if more patchiness is more realistic/is what in reality happens in the catchment?

REPLY: Dear reviewer with field photographs and high resolution satellite images it was clear that we have heterogeneous snow-cover in our catchment, especially during the melting melting period. That is why this whole study is designed to find the algorithms to better simulate the heterogeneity in snow-cover. We will keep your suggestion while rephrasing this sentence.

-Line 28: spatial differences in melting

REPLY: We noted this correction.

-Line 30: please be specific. Change the hydrology in which sense? Please revise.

REPLY: It should be “hydrological fluxes”.

-Line 35: please revise the first sentence. What does this sentence mean?

REPLY: We will rephrase it as “However, hydrological flux exchange between surface and subsurface in LSMs is often poorly constrained”.

-Line 40: what does “proper soil moisture” mean? Please revise this sentence.

REPLY: It should be “they failed to simulate the heterogeneous soil moisture compared to observation”.

-Line 40: sloppy? Do the authors mean sloping?

REPLY: Sorry for this mistake, it should be sloping and we will correct it.

-Lines 41-42: I do not understand this sentence: precipitation, etc. can simulate the spatial variability in hydrological fluxes? Please revise.

REPLY: We will rephrase it as “Similarly, another study acknowledged that precipitation, solar insolation and wind distribution in a hillslope catchment are vital to simulate the spatial heterogeneity in surface hydrological fluxes and snow dynamics (Sun et al., 2018)”.

-Line 43: please replace “coverage” with “cover” (also in the whole manuscript).

REPLY: Dear reviewer thank you for this highlight, we will change it at every instance.

-Line 43: what does limit mean? Please revise this sentence. What does “differential melting” mean? Please revise.

REPLY: Instead of “limit” it should be “under representation”. And, it should be “heterogeneous snow melting”.

-Line 45: please revise “differential snow melting”.

REPLY: We will change it with “heterogeneous snow melting”.

-Line 45: what does “variable saturation” mean? Please revise.

REPLY: It should be “spatial variation in saturation and pressure head”

-Line 47: “snowy catchment”?

REPLY: We will correct it as “snow dominated catchment”.

-Lines 52-53: Shortwave? Do the authors mean shortwave radiation? Please revise this in the whole manuscript.

REPLY: We will correct it at every instance.

-Line 54: please explain here what directional effects mean.

REPLY: It should be “terrain, wind speed and soil moisture”, we will change it.

-Line 56: “meteorological distributions”? Please revise.

REPLY: We replace it with “forcing distribution of single variable”.

-Line 60: please add what physical processes are meant here. Please be specific.

REPLY: We will rephrase it as “However, these diverse approaches in hydrological modeling are still limited and merely account for subsurface distribution, hyper-resolution simulation, terrain effect and surface meteorological variable distribution.”

-Lines 61-62: the meaning of these sentences is not clear, please revise.

REPLY: We will revise this sentence as “In mountainous regions it is hard to maintain a dense network of weather stations due to the complex terrain.”

-Line 64: “meteorological parameters”? Please revise.

REPLY: We replace it as “meteorological variables like precipitation, shortwave radiation, wind speed, temperature and humidity over the catchment”

-Line 65: catchments

REPLY: We noted this correction.

-Line 64: please replace “catch” with simulate or reproduce.

REPLY: We will change it at every instance.

-Line 71: on the water balance (or budget)

REPLY: It should be “balance”.

-Line 72: spatially distributing precipitation

REPLY: We noted this correction.

-Line 77: based on the manuscript I am a bit confused: the model was not validated; and it is also unclear for which observations the model was calibrated.

REPLY: As stated we have calibrated our model against the proxy variables like albedo, eddy covariance and snow cover.

-Line 79: what does individual or combined mean here? Can the authors please provide some explanation?

REPLY: We will rephrase this sentence. Individual mean when only one variable is distributed and combined means when all variables are distributed together.

-Lines 79-80: these goals are not clear, please explain or revise/rephrase.

REPLY: We will rephrase this sentence as “Impact of precipitation, solar radiation and wind speed distribution over catchment; and its response in simulating the catchment hydrological fluxes.”

-Line 83: details

REPLY: We noted this correction.

-Line 83: “meteorological distribution”? Please revise.

REPLY: We will replace the sentence as “This section also includes details about the method to distribute the meteorological variables.”

-Line 83: The fourth

REPLY: We noted this correction.

-Line 91: please add: 5 to 6 months per year

REPLY: We will correct it according to your suggestion.

-Line 91: This sentence is unclear. What is “C4”? What does this sentence mean? Please revise.

REPLY: This will be revised as “The warm season grassland dominates the summer with 5 % woody coverage”

-Figure 1: caption: please remove second “for” from fourth line. Snow coefficients and wind direction mask: how were these derived? Can the authors either explain in the caption or refer to the main text where it is explained? Otherwise it is very hard to understand the figure.

REPLY: We have noted the correction. Snow coefficients are explained in “precipitation” subsection of meteorological distribution section. Wind direction mask is explained in method section now.

-Line 98: Please consider to revise, e.g. The study area is located in a mid-latitude…

REPLY: We will change it as “The study area is located in a typical mid-latitude alpine climate”.

-Line 99: on 11 November

REPLY: We noted this correction.

-Line 105: “are well phased” – what does this mean? They are in phase? Please revise this sentence.

REPLY: It will be revised as “Temperature and specific humidity follows the same cyclic pattern.”

-Line 107: what does “also” mean? What other purpose did the observations serve?

REPLY: We will rephrase it as “These observations time series are used as the input to force the model”.

-Line 108: input time series?

REPLY: We will rephrase it as stated above.

-Line 111: wind speed

REPLY: We noted this typing correction.

-Line 115: Please revise the first sentence. In 2017 an OTT Pluvio rain gauge was installed at the weather station?

REPLY: Dear reviewer thank you for pronouncing the correct statement, we will keep your suggestion.

-Line 117: what does “reduced to a 30 min time step” mean? Averaged? Please revise.

REPLY: We will revise this sentence. It is basically the sum for precipitation and mean for all other variables. We rephrased it new text.

-Line 124: what does “solve the surface and subsurface flow” mean? Please revise.

REPLY: We will change it as “solve the surface and subsurface exchange of fluxes”.

-Line 125: transfers of what? Please add.

REPLY: It should be “transfer of fluxes”. We will revise it.

-Line 128: make

REPLY: We noted this correction.

-Lines 128-129: “grid which eases boundary conditions prescription mesh refinements” I am not entirely sure what the authors mean here.

REPLY: This is Parflow terminology and we will rephrase it as “ParFLOW includes a terrain-following grid which eases boundary conditions prescription. It accounts the surface slope in the Darcy’s formula which also eases numerical exchange between subsurface and overland flow.”

-Line 130: kinematic

REPLY: We noted this typing correction.

-Line 130: “any saturated cell flows” please revise.

REPLY: It should be “all saturated cell flows”.

-Line 135: Common Land Model

REPLY: Thank you for highlighting this mistake. We have noted the correction.

-Line 140: snow layer thickness? Or snow depth?

REPLY: This is a CLM scheme and it divides the total snow depth into 5 snow layers.

-Line 142: “snow fraction is used to account for the surface uncovered by snow” I am not entirely sure what the authors mean here.

REPLY: It should be “snow fraction is used to calculate the total snow cover area”. We will correct it.

-Line 143: please explain how this reduction was exactly done.

REPLY: This was done because of hyper-resolution modeling framework. We will rephrase it as “In our study snow fraction was assigned to 0 (no-snow) or 1 (snow) values. Our horizontal pixel resolution is small enough (10 × 10 m) that we consider their snow cover to be uniform. This implies that either our pixels are completely covered with snow or they are bare.”

-Line 144: cover

REPLY: We noted this correction.

-Line 145: temperature

REPLY: We noted this correction.

-Line 148: Further information on…

REPLY: We will keep your suggestion.

-Line 155: please explain how these factors were derived/where they come from.

REPLY: In line 151 we put the reference and all the parameters were adopted from that study. We will repeat the citation in the following line as well.

-Line 162: please explain how this upscaling was done.

REPLY: Upscaling was done “using the nearest neighbour algorithm”. We will specify it in the sentence.

-Line 167: It must be noted

REPLY: We noted the correction.

-Lines 169-170: I am not sure what exactly this means: “rain has not been distributed according to an altitudinal gradient”. Does this mean that precipitation in terms of rain was not distributed over the catchment, but only snow?

REPLY: Yes, we assume that rain falls homogeneously in our microscale catchment and wind does not impact the liquid precipitation. However, solid precipitation gets impacted before and after its deposition from wind. And, snow blowing through wind is a common phenomenon in our catchment due to strong wind. Hence, we only distribute precipitation when it falls as snow.

-Line 172, 174, 194, 201, 233: shortwave radiation

REPLY: We noted this correction

-Eq. 3 and 4: cosi?

REPLY: Yes, it is cosi

-Line 183: was

REPLY: We noted this correction.

-Line 201: air pressure?

REPLY: It should be atmospheric pressure, we will include it.

-Line 203: please explain how, or provide a reference.

REPLY: This will be revised as "These observations data were available at 30 minutes interval from the instruments. The data which were available at a higher temporal resolution were upscaled to the 30 minutes temporal resolution using mean and sum approach."

-Line 209: which satellite images, and how was this done exactly? Please explain.

REPLY: This is not the satellite image and has been done only over the catchment area. We will add an explanation about it.

-Line 213: allows

REPLY: We noted the correction.

-Line 219: what does "key profile" mean? Where exactly? Or how were these locations selected? How many locations? Please be specific, so that the results are reproducible.

REPLY: Dear reviewer we will elaborate it. However, the detail explanation will be provided through our companion manuscript. Our companion manuscript will be more dedicated towards subsurface sensitivity and model calibration.

-Line 222: what does "meteorological distribution" mean? Please revise.

REPLY: It should be meteorological variable distribution. We will correct it at every instance.

-Figure 3: is it maybe possible to please replace the colour name "salmon" with e.g. pink or light red? In third line: please remove "from" and replace it with e.g. in terms of

REPLY: Dear reviewer we will keep your suggestion and replace the colour with pink and dark brown.

-Line 223: what does "outputted" mean? Was the simulation time step one hour? Or just the results were written out for every hour? Please explain.

REPLY: Simulation time step is 30 minutes and outputs were written at every hour (24 outputs in one day). We will change the sentence as "The model has been forced with half-hourly meteorological forcing, however results were written at hourly time-step".

-Line 225: what does "stock difference" mean? Please revise.

REPLY: It should be "storage difference".

-Line 226: please remove "to"

REPLY: We noted the correction.

-Line 226: the 10^{th} year

REPLY: We noted the correction.

-Line 228: spatial distribution of only (similarly in the next lines)

REPLY: We will modify it according to your suggestion.

-Line 235: mean

REPLY: We noted the correction.

-Lines 238-239: Please revise this sentence.

REPLY: Yes, we will rephrase it to sound more clear.

-Line 240: please explain why.

REPLY: This happened because of our shortwave radiation distribution algorithm. Our distribution scheme accounts the slope and hillshade zone. Our catchment is has a very undulating terrain hence, the shotwave radiation on average is reduced. We will explain it along the description.

-Line 240, 241: "-2" should be in the exponent.

REPLY: We noted this correction.

-Line 247: serve

REPLY: We noted this correction.

-Line 247: "catching the slope, curvature and aspect effect in spatial distribution" – Please revise.

REPLY: Dear reviewer lines 264 to 285 lines are properly revised in manuscript to sound more clear.

-Line 249: variation between what? Please add. Please explain how the authors obtained these numbers.

REPLY: Dear reviewer we added a complete paragraph in revised manuscript to sound it more clear (line 278-285).

-Line 253: accumulation of what? Please add.

REPLY: It should be "snow accumulation period". We will add it.

-Line 258: what is a "subsurface stock"? Storage? Of what?

REPLY: It should be "storage". We will change it at every instance.

-Figure 4.a.: legend missing. c: shortwave radiation? c, d: over the watershed (instead of along)

REPLY: 4(a) Speed and direction both legends are included in the figure. 4(c) it should be "shortwave radiation". 4(c) (d) we will keep your suggestion.

-Line 262: variability of what? Please add.

REPLY: It should be "snow variability".

-Line 264: what is a "subsurface stock"? Storage? Of what? Please revise.

REPLY: It should be "storage". We will change it at every instance.

-Line 265: please avoid referring to figures which are somewhere else/come much later in the results. Figure 9 is not yet explained. This would be very confusing for a reader.

REPLY: Dear reviewer we will keep your suggestion and rephrase the sentence.

-Line 266-267: this sentence is grammatically incorrect, please revise.

REPLY: YES, we will revise this sentence. It should be "The net radiation contributes to snowmelt in early spring. The factors responsible for this phenomenon includes higher sun elevation, clear sky conditions and higher daily temperature.

-Line 269: cover (also later).

REPLY: We noted this correction.

-Line 271: in the extraction

REPLY: We noted this correction.

-Line 273: “subsurface stock increment” What does this mean? Please revise.

REPLY: It should be “small increase in subsurface storage”.

-Line 273: when ET is smaller?

REPLY: We would like to keep it as “when ET decreases”

-Line 274: this sentence is unclear. Please revise.

REPLY: We will revise this sentence as In the end 310 of the hydrological year, the subsurface water storage has a deficit of 0.15 mm which is much smaller than the annual cycle amplitude.”

-Line 276: difference between what and what? Please add.

REPLY: It is “major difference compared to 5a ,b”. We will correct it.

-Line 277: were prescribed. Please use consistently either present or past tense when showing the results, please avoid mixing.

REPLY: We will change it as present tense (are).

-Figure 5: legend: storage and not stock, please revise.

REPLY: We noted the correction.

-Line 280: several years? Please explain.

REPLY: It should be 10 years.

-Line 281: orientation of what? Please add.

REPLY: It should be “catchment orientation”.

-Line 284: fluxes of what? Please add.

REPLY: We will change it as “surface hydrological fluxes”.

-Line 291: are they the same or not? Please revise.

REPLY: The difference is runoff between these two simulation “is only 2%”. We will specify this.

-Line 291: distribution of what? Which scale? Please add.

REPLY: It should be “at scale of annual water budget”. We will rephrase the whole sentence.

-Paragraph starting in Line 293: this seems to be the very same paragraph as the one before (just being incomplete).

REPLY: Dear reviewer this is the typing mistake from our side. We are sorry for this and we will remove the subsequent paragraph.

-Line 299: seem

REPLY: We noted the correction.

-Line 302: produces spatial variability in snow melt

REPLY: We will keep your suggestion.

-Line 303: please remove comma

REPLY: We noted the correction.

-Line 305: larger than

REPLY: We noted the correction.

-Line 305: “-1” should be in the exponent

REPLY: We will correct it.

-Line 308: what does “marked on” mean? Please revise.

REPLY: It should be “The impact of the late April and early May rain-on-snow period is visible on streamflow.”

-Line 310: please explain.

REPLY: We will revise the whole sentence as “Unsurprisingly 2D-WD and 2D-PD simulations show larger streamflow values compared to 2D-AD and 2D-SD simulations. This happens because for the former two, the catchment receives 38.7 W m^{-2} less radiation than the latter two. Concerning the summer period when snow gets melted, these differences are not visible on the streamflow.”

-Line 310: than the two latter

REPLY: We noted the correction.

-Line 308: It must be noted

REPLY: We will keep your suggestion.

-Line 311: is there any difference or is there no difference? Please revise this sentence.

REPLY: We will change it as said above.

-Line 314: please explain in methods how the footprint area was exactly estimated.

REPLY: We will include this to methods section (3.3)

-Figure 7 caption: “Evapotranspiration simulation masked with wind direction mask for 17 days” Please explain this in detail in methods.

REPLY: Wind direction mask is explained in section 3.3 now.

-Figure 7: ET is overestimated by each simulation. Why is that? Would it be possible to e.g. adjust model parameters?

REPLY: We have explained this in detail in the specific comments section.

-Line 319: saffron curve – orange curve?

REPLY: Sorry for this mistake, this should be orange.

-Line 321: “d-b” – does this mean b, c and d? Please revise.

REPLY: It should be 7b, d. It only references 7b and 7d, we will correct it

-Line 321: “The cause is that the average shortwave after the distribution is less than the shortwave without distribution” I am not sure what the authors mean here. Please revise this sentence.

REPLY: We have explained this in first section of the result. We will rephrase this sentence as “This happens because the average shortwave radiation after the distribution is less than the shortwave radiation without distribution (section 4). Also, the catchment is facing east which actually reduces direct incoming solar radiation from noon to sunset.

-Line 324: overestimate

REPLY: We noted the correction.

-Table 1: “Stock”? Storage? Of what, where?

REPLY: It should be storage, we will correct it at every instance.

-Line 326: “To the first order”? Please revise.

REPLY: Thank you for highlighting this, it should be “However”.

-Line 327: Shortwave radiation

REPLY: We noted the correction.

-Line 329: what does “correspond much better” mean? Please quantify this.

REPLY: We will put the mathematical numbers as “(decrease in regression slope from 1.55 to 1.18).”

-Line 330: water balance?

REPLY: We will change it as hydrological budget.

-Line 337, 339, 342: stock?

REPLY: It should be storage.

-Line 341: “ET drawdown subsurface stock” – Please revise.

REPLY: It should be “ET decreases the subsurface storage”.

-Line 344: dynamics

REPLY: We noted the correction.

-Line 351: “return to its snow value” – Please revise

REPLY: It should be “return to its maximum snow albedo value”.

-Line 354-355: please remove - figure caption should describe what is on a plot, in main text just please refer to a figure.

REPLY: We will keep your suggestion.

-Line 356, 361: cover

REPLY: We will correct it.

-Line 357: accumulation of what? Please add.

REPLY: It should be “snow accumulation”, we will correct it.

-Line 358: which other ones? Please be specific.

REPLY: It should be “but not on any other distributed forcing simulation”.

-Figure: there seems to be two red lines on the figure, please choose a different colour for e.g. Pix-PM albedo. A: right blue axis title for precipitation is missing. Please add dimension to each axis title - If dimensionless then simply “(-)”.

REPLY: Dear reviewer thank you for highlighting this issue. We have changes is as “yellow”.

-Lines 371-373: this belongs to the methods section.

REPLY: We will keep it methodology section along with wind direction mask (section 3.4).

-Table 2: belongs to the methods.

REPLY: We will shift it to the methodology section along with description (section 3.4).

-Line 378: “The 2D-AD simulation has less green pixels” Please revise, and add physical explanation.

REPLY: It should be “less snow cover”, we will rephrase it.

-Line 383: “The 25th of May is located when snow partially cover the catchment during the melting period.” Please revise this sentence, the meaning is unclear.

REPLY: We will change it as “On 25th of May the catchment is partially snow covered which is specific to the advancement of melting season”.

-Line 383: represents

REPLY: We noted this correction.

-Line 388: what does “slightly” mean? Please revise, please be specific. How late exactly?

REPLY: We have revised the whole paragraph with mathematical estimation.

-Line 391: what does “play” mean? Please rewrite this sentence.

REPLY: We will rephrase this as said above.

-Line 394: Overall

REPLY: We noted this correction.

-Line 396, 405: cover

REPLY: We will change it.

-Line 397: catchments

REPLY: We noted this correction.

-Please add a Discussion section to the manuscript.

REPLY: We will put a separate discussion section in the manuscript (section 6).

-Line 417: spatially distributed snow melt?

REPLY: Differential snow melting will be more appropriate for this sentence.

-Line 409: “snow-stays”? – days with snow cover?

REPLY: it is “more than a month”

-Line 409: “These longer snow-stays lead to a significant impact on the hydrological cycle from increased water storage to evapotranspiration regime” – please revise this sentence.

REPLY: We will revise it as “These longer snow stays lead to increase in streamflow, subsurface water stor- age and runoff coefficients but decrease in evapotranspiration because of shorter vegetation periods.”

-Line 411: “because it favours the appearance of no-snow patches in the melting season” Please revise.

REPLY: It should be “because it favours the appearance of snow patches during the melting season”.

-Line 411: Shortwave radiation

REPLY: We noted this correction.

-Line 412: “differential melting”? Please revise.

REPLY: It should be “differential snow melting”.

-Line 414: reduced

REPLY: It should be “reduces”.

-Lines 417-418: please revise this sentence.

REPLY: It should be “Furthermore, accounting for distributed solar incidence reduces the incoming radiation in our catchment which subsequently reduces the evapotranspiration.”

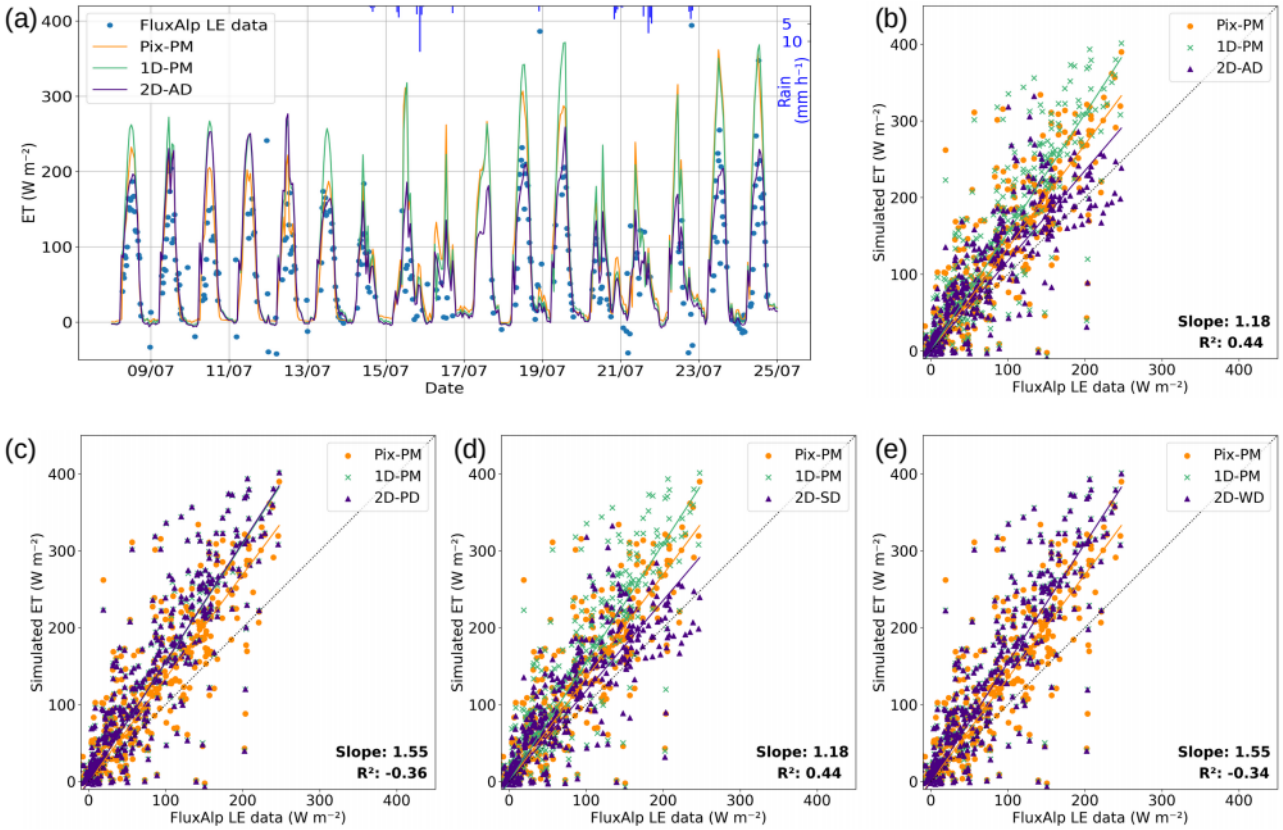
-Line 421: dynamics

REPLY: We noted this correction.

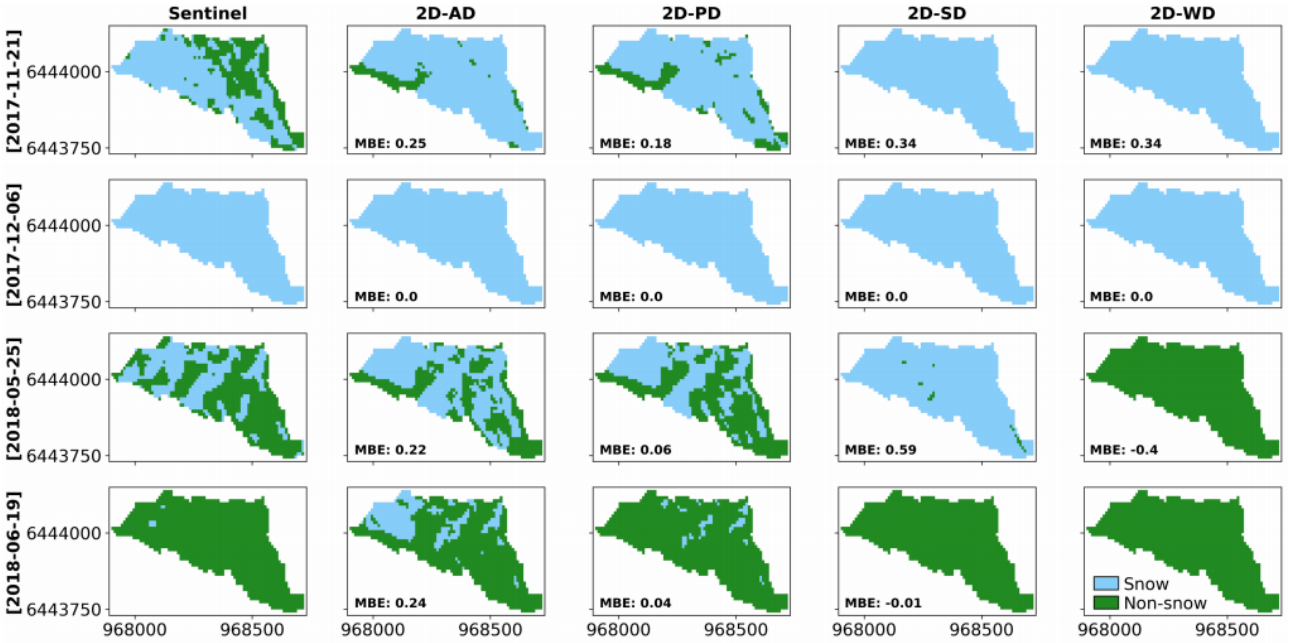
-Line 421: “has to be accounted accordingly for hydrological processes” – do the authors here mean accounting for the terrain? Please revise.
REPLY: It should be “accounted using terrain based meteorologic forcing distribution”.

Review Table 1: Statistical metrics for observed and simulated parameter among different simulations.

Variable	**Metrics**		**2D-AD**	**2D-PD**	**2D-SD**	**2D-WD**
Evapotranspiration	Slope		1.18	1.55	1.18	1.55
	R-Square		0.44	-0.36	0.44	-0.34
	RMSE		50.77	79.14	50.90	78.41
Albedo	R-Square		0.85	0.88	0.77	0.85
	RMSE		0.12	0.10	0.14	0.12
Snow cover (Sentinel2)	MBE	21 Nov, 2017	0.25	0.18	0.34	0.34
		06 Dec, 2017	0.00	0.00	0.00	0.00
		25 May, 2018	0.22	0.06	0.59	-0.40
		19 June, 2018	0.24	0.04	-0.01	-0.01
	RMSE	21 Nov, 2017	0.63	0.65	0.58	0.58
		06 Dec, 2017	0.00	0.00	0.00	0.00
		25 May, 2018	0.74	0.75	0.78	0.63
		19 June, 2018	0.50	0.23	0.07	0.07



Revise Figure 7: (a) Evapotranspiration simulation masked with wind direction mask for 17 days in summer for all distributed run (2D-AD). Scatter plot in the same month for (b) all distributed run (2D-AD), (c) only precipitation distributed run (2D-PD), (d) only shortwave radiation distributed run (2D-SD) and (e) only wind distributed run (2D-WD).



Revise Figure 10: Snow map for different simulations compared with the Sentinel-2 images for 4 cloud free images: snow pixels (light skyblue) and non-snow pixel (green).

Review Table 2: Distributed and non-distributed approach adopted for different simulation.

	Precipitation	**Shortwave radiation**	**Wind speed**
Pix-PM	Distributed mean	Non-distributed	Non-distributed
1D-PM	Distributed mean	Non-distributed	Non-distributed
1D-AM	Distributed mean	Distributed mean	Distributed mean
2D-AD	Distributed	Distributed	Distributed
2D-PD	Distributed	Non-distributed	Non-distributed
2D-SD	Distributed mean	Distributed	Non-distributed
2D-WD	Distributed mean	Non-distributed	Distributed